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In the study of educationally disadvantaged pre-school children, 160 predominantly Negro children 3½ to 4½ years of age were pretested on a battery consisting of a language test, designed to explore the children's fantasy behavior, and two brief intelligence tests. Children were assigned to nursery and non-nursery groups on the basis of language, age, and sex. The nursery children had six weeks of preschool experience. All the mothers participating in the experiment were Negroes. Post-testing was carried out four weeks after the summer nursery was completed, and involved administration of the entire battery used in pre-testing. Half of the children would receive follow-up experience from an educator and a social worker, while the other half of the children would not receive such follow-up experience.

The initial uncooperativeness of some mothers had a definite effect on the children's test performance. Many were extremely intimidated and overwhelmed in their first encounter. The mothers' attitude toward the situation also influenced mother-child interactions.

The basic differences between the mothers in the study and other mothers suggest that differences in the maternal behavior between middle and lower-class mothers had to be examined not only in the light of class differences, but also in terms of differences in the meaning of the experimental situation to the subject.

The sampling restrictions on the number of variables which could be systematically studied emphasized the importance of knowing for future research which variables were essential and which were relatively unimportant for a population of this kind. In the future it would seem necessary to use variables such as a child's reaction to the initial test situation rather than language age, for purposes of stratification.

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This paper was presented on February 12, 1965 at the Annual Meeting of the American Educational Research Association in Chicago, Illinois.

RESEARCH WITH EDUCATIONALLY DISADVANTAGED PRE-SCHOOL CHILDREN

By E. KUNO BELLER and ALLAN NASH

TEMPLE UNIVERSITY

Our thoughts and comments that we are about to share with you today are based on two of our research projects. The first of these studies started last May as part of a pre-school project carried out by the Department of Public Instruction of the Commonwealth of Pennsylvania. Our research was limited to two locations of this project, that is, Chester, Pennsylvania and Harrisburg, Pennsylvania. In the study one hundred sixty predominantly Negro children 3 1/2 to 4 1/2 years of age were pretested on a battery consisting of a language test, a test designed

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The present report is based on two pre-school projects with educationally deprived children. One of these two projects is being carried out by the Department of Public Instruction of the Commonwealth of Pennsylvania under the educational direction of Drs. Allen Hartman and Evangeline Ward, and Mr. Ernest Rookie, with joint support from the Commonwealth of Pennsylvania and a grant from The Ford Foundation. The other project is being carried out by the Philadelphia Council for Community Advancement, a Ford Foundation-funded agency, and the Public School system of the City of Philadelphia under the educational direction of Mr. David Horowitz and Mrs. Gabrielle J. Faddis.

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to explore the children's phantasy behavior, and two brief intelligence tests. Children were assigned to nursery and non-nursery groups on the basis of language, age and sex, in a stratified random selection procedure. The nursery children had six weeks of preschool experience. (During that time measures were obtained on the children's social behavior in the nursery, and in addition, measures of maternal behavior were obtained in a mother-child interaction experiment which was carried out with eighty mother-child pairs from both the nursery and non-nursery groups.) All the mothers participating in this experiment were Negroes. Post-testing was carried out four weeks after the summer nursery was completed, and involved administration of the entire battery used in pre-testing. Following post-testing the nursery and non-nursery groups were divided once more in such a way that half of the children would receive follow-up experience from an educator and social worker while the other half of the children would not receive such follow-up experience. It is contemplated that all one hundred sixty children will be re-tested again in June of this year with the entire battery that was used both in the pre-testing and in the immediate post-testing.

The second study on which our comments are based today started in October 1964 as part of a joint pre-school project of the Philadelphia Council of Community Advancement and the Philadelphia Public

School system. This project encompasses eight pre-school classes attended by approximately one hundred thirty children with a non-nursery group of sixty-five control children. In this particular study the parents were first interviewed and their children assigned randomly to nursery and non-nursery groups without any pre-testing preceding such assignment. Six weeks after the nursery started we administered a language test and a brief intelligence test to ninety-six of the pre-school children in the Philadelphia Project and to forty-eight control children. Finally we are currently engaged with Miss Ida Kravitz in a cognitive and language development project with the Philadelphia children. This much as a background for our comments.

One of our outstanding impressions in the beginning of our studies was the striking unevenness in the response of mothers to appointments made with them for bringing their children for testing. Some of the mothers kept the appointments, but many more needed repeated reminding after they failed to show up at the set time. The inability of these mothers to cooperate was due to many different reasons, ranging from disorientation in time and a mixture of suspiciousness and lack of interest in the project, to practical considerations such as having to go and pick up Relief, not having shoes for the child to put on to take him to the testing situation, having sick children at home, etc. These adverse conditions and the added incentive that became

necessary to get the parent to bring the child for testing already have important implications for systematic and planned research. First, it takes more staff and therefore a good deal more money to carry out research with this type of population than with children coming from middle-class or from lower-middle-class homes. The lack of such hindsight, or foresight, made it necessary for us to use up resources planned for other phases of our research just to make the very beginning of our systematic study possible without fatal consequences for our design. Imagine what would have followed if we had thrown up our hands and decided to work only with those parents who were cooperative and whom we could manage to include in our pre-testing within the limits of our planned budget. We not only would have ended up with a smaller sample, but more importantly with a biased sample. Our study would have been limited to children of cooperative parents or parents who were under less economic stress, and of children from more orderly and less chaotic homes. Such biased sampling would have imposed considerable limitations for generalizing from our study.

A further consequence of the uncooperativeness of our parents is the effect this factor might have on the child's performance during the testing situation. For example, we noticed that the children of mothers who appeared hostile manifested greater anxiety and resistance in entering the test situation. Parenthetically, this is an

impression, not a finding. Moreover, the research team exerted a great deal more effort and attention in bringing the uncooperative and unmotivated parent to the test situation. This effort and attention, while quantitatively comparable, varied considerably in quality from mother to mother. For example, we would spend the same amount of time and effort with two different mothers; however, the approach with one mother might be gentle prodding, while the approach with the other might be greater insistence, more urgency and the like. This difference in the quality of attention was of course a function both of the particular member of the research team as well as the particular parent involved. We are not certain that these differences in the amount and quality of effort and attention expended on different mothers in our sample actually had an effect on the child's performance. However, these differences were so marked that it certainly would be indicated that some effort should be made to control this factor more carefully in further research with this type of population. This whole problem of cooperation and incentive will be taken up in a few moments again when we discuss its implications for our study of mother-child interaction.

How did our children react to the testing situation? Many were extremely intimidated and overwhelmed in their first encounter with this type of situation. Quite a few children were so inhibited that

they failed to respond to a good many questions. It is clear that under such circumstances a test of a child's performance is probably not a valid measure of his true ability.* When comparing the average performance of a group of middle-class children it is important to keep in mind that the factors we have mentioned could depress the performance level of lower-class educationally-deprived children much more than the performance level of middle-class children. This conclusion would apply particularly to test scores based on performances of children from ethnic minority groups in unfamiliar situations in which the examiner was a stranger to the child.

Another consequence of this situation might be that intermittent nursery experience which is primarily permissive might actually temporarily depress the performance of certain children on the post-test measure. This would be particularly true for a hyperactive child who, because of his awe for the unfamiliar authority situation, exerted a great deal of effort in following instructions which increased his attention span and actually improved his perform-

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* However, it is probably quite a valid measure of a child's true functioning level in a formal educational situation in which intellectual demands are made upon him.

ance. When he finds out in the nursery situation that he does not get punished for ignoring instructions he might be less inclined to follow instructions, try less hard and perform at a lower level if tested or retested during this initial phase of his nursery experience.

What are the answers to this problem? Should we wait and not give the tests until after the child has been in the nursery for approximately one or two weeks? It is our opinion that such an approach would produce uncontrolled and unmeasured differences in the pre-test data between the experimental and the control groups, that is, between children who have been in the nursery and the control group children who are not entering the nursery until a later date. An alternative solution of this problem is to study variability in the attitudes and emotional reactions of children to the initial test situation and the consequences of such variability on test performance. In our own study we have introduced a set of rating scales which were used consistently in every single test situation. These scales deal with inhibition, tension, degree of involvement in the task, etc. With the aid of these measures we hope to determine the effects of emotional and motivational factors on a child's initial test score as well as on the change from pre- to post-testing.

As I indicated earlier, the issue of the mother's cooperation and her attitude towards this situation posed an even more serious problem for our experiment on mother-child interactions than for the pre-testing of her child. During the summer we replicated a study which was previously carried out with middle-class white mothers and in one instance with lower-class white mothers (Merrill, Bishop, Sholluck, Walters and Zunich). Our group consisted of lower-class Negro mothers coming from highly deprived urban slums. Briefly, in this experiment all mothers were observed for two half-hour periods. For the first observation period every mother was given the instruction that we were interested in studying children when they play with their mothers. We asked each mother to pretend that she was finished with her house work and now had half an hour to devote to her child. Prior to the second observation half of the mothers were told that we had expected their child to do better than he did, that probably the child had been unfamiliar with the situation the first time and now since he was more familiar with the situation we expected him to do better on the second occasion.

Although we have only completed the first of several steps in the analysis of this experiment, it is by now clear that our

study will yield different findings than those which emerged from all the other studies carried out previously. In all other studies the effect of the experimental instructions was that the mothers tried harder to influence the child, and this manifested itself in increase of such behaviors as directing, helping, teaching, etc. We find no such trend. If anything, our mothers appeared to become less cooperative and to withdraw following the experimental instruction. We are not here concerned with the substantive finding itself, but rather with the implications these findings might have for doing research with this type of population.

To begin with, several mothers told us right out that they had never spent half an hour alone with their child. Though willing, they felt awkward and strange in fulfilling our request. I suspect that even though only a few mothers made such statements explicitly and spontaneously, these statements applied to quite a few of our mothers. This is a striking difference between our mothers and the mothers in the other studies. As mentioned earlier it is very probable that the variability in the cooperation displayed by our mothers would effect the functioning of a mother in our experiment. It is not likely that the mothers in the other studies exhibited such wide range of differences in their cooperation.

Thus our mothers may well have differed from those in other studies in certain basic respects, i.e. the degree of their familiarity with the experimental task, their readiness to enter the situation and the amount of prodding and persuasion necessary to enlist their participation.

The basic differences between the mothers in our studies and others suggest certain conclusions for further research. They suggest that differences in the maternal behavior between middle- and lower-class mothers have to be examined not only in the light of class differences, but also in terms of differences in the meaning of the experimental situation to the subject. A mother who is brought to the experimental situation only with much effort is also likely to perceive herself as passive, and the experimenter may appear to her initially as a supporting figure. If this assumption is true, then the experimental instruction might be expected to have quite a different effect on the mothers in our study than it would on the mothers in the other studies. A middle-class white mother is likely to enter the experimental situation much on her own will, and to take more responsibility for her interaction with her child in this particular situation. To tell such a mother that "her child could do better than he did" when alone with her will function as a challenge to the mother. However,

once a mother perceives herself as passive, and the researcher as supportive, then a statement by the researcher that the child has not done as well as expected and could do better when alone with his mother and entrusted to her, is more likely to function as a form of punishment than as an incentive to the mother. I wonder to what extent the impact of the research situation on the lower-class Negro mother has contributed to the finding that these mothers tend to be more punitive and more authoritarian in their interaction with their children.

On the positive side, we feel that the factors which we have enumerated can be controlled and fairly easily manipulated in further research. Therefore, the issue that we have raised can be settled by facts rather than having to be continued as speculations.

Finally, we would like to raise some of the methodological questions which we encountered in this research, and which we feel are of general interest.

Our investigations, like most, operated under practical restrictions. We were able to study only a fraction of the variables which might be of interest in evaluating a nursery program, or which might illuminate the characteristics of this special population.

As indicated earlier, in the summer program we were able to

classify our subjects in the nursery and non-nursery groups by sex and degree of language development, but there are admittedly many other dimensions along which we might have profitably stratified. Our decision to limit the number of subject classifications in the design was based on the fact that, even with 160 subjects, we found the sample sizes to approach the lower limits for meeting certain statistical assumptions after grouping the children into smaller sub-classifications.

The sampling restrictions on the number of variables which can be systematically studied emphasize the importance of knowing for future research which variables are essential and which are relatively unimportant for a population of this kind. As you will have gathered by now we shall be more inclined to use variables such as a child's reaction to the initial test situation rather than language age for purposes of stratification.

There is one other consideration which we feel requires some attention, and this is the distinction between statistical significance and practical significance. In evaluating the effectiveness of any program, it is probably not desirable to interpret the data solely by reference to a statistical level of significance. While statistical tests are essential for testing certain hypotheses objectively it is equally important, for practical purposes, to pay attention to the magnitude of the experimental effects. This dist-

inction becomes more important as our research gains sophistication in the techniques of control and experimental design. The increased precision makes it possible to detect smaller differences between populations, smaller relationships among variables, and smaller gains of experimental subjects.

To illustrate, a new educational program may show statistically significant gains for a group, but of such a small degree that it actually affords little practical advantage. For example, an increase of a few IQ points may be of questionable value, particularly when the cost of training teachers and changing facilities is extremely high.

Practical considerations may shift the emphasis in the opposite direction. Relatively small beneficial effects may represent a significant achievement if the skills involved, such as language and reading, are essential for a child to profit from his formal education. Similarly, several small changes in attitude, motivation and skill may combine to produce a common larger effect on a child's total response to educational programs.

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